ROYAL ASTRONOMICAL SOCIETY.

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G. B. AIRY, Esq. President, in the Chair.

Wadham Lock Sutton, Esq., Berkhampstead, Herts, was balloted for and duly elected a Fellow of the Society.

Vol. xxiii. of the *Memoirs* has recently been published. It contains three plates of engravings, besides a woodcut illustration accompanying one of the papers. The price of the volume is 4*s.* to Fellows of the Society and 8*s.* to the public. Vol. xiv. of the *Monthly Notices*, which contains shorter communications, abstracts of papers, lectures, &c., is given gratuitously to purchasers of the quarto volume. The two publications are supplementary to each other, and are to be considered as parts of the same series. They contain scarcely anything in common, except the Annual Report; and between them include a complete account of the proceedings of the Society during the year.

On the Construction of New Lunar Tables, and on some points in the Lunar Theory depending on the Conformation of the Moon with respect to its Centre of Gravity. By Professor Hansen.

(*Letter to the Astronomer Royal.*)

"As regards the Lunar Tables, I am now so far advanced with their construction as to be in possession of tables which represent the individual observations of the moon with a great degree of accuracy, and leave hardly any greater outstanding differences than those which are usually met with in observations of the fixed stars. After the correction of the originally assumed elliptic elements and of those coefficients of perturbation, the values of which can be ascertained *only* by observation, the following Greenwich observations were compared with the tables. In this operation there was no selection of the observations, those actually employed having been arbitrarily assumed beforehand, without knowing what might be the result of the comparison. Moreover, no further comparison of Greenwich observations with the tables has been instituted *subsequently* to the correction of the elements, so that the following results are unaccompanied by any circumstance which might tend to produce a bias in their favour.

"The outstanding differences between the observations and the tables (theory—observation) are the following, in which α denotes the right ascension and δ the declination.

Date.		$\Delta\alpha \cos\delta$.	Remarks.		$\Delta\delta$.	Remarks.
		″			″	
1824, Mar.	8	0·0			+1·1	
	9	−2·9	...	...	−0·6	
	12	+0·2	...	...	+6·8	
	13	+2·1	...	...	−0·8	
	14	−0·9	...	...	+5·8	
	16	+2·1	..	...	−1·7	
	19	−0·6	...	...	−0·7	
Aug.	31	+4·7	...	...	+0·8	
Sept.	1	+0·5	...	...	−2·5	
	2	+0·7	...	...	−0·6	
	4	−3·2	...	...	+2·4	
	5	−1·5	...	...	...	
	6	−3·5	...	...	−1·5	
	8	−3·5			−5·2	
	8	−3·9	...	...		
	12	+5·4	...	...	+4·6	
	13	+2·7	...	...	+1·5	
	15	+0·7	...	...	+0·4	
1832, Mar.	8	−6·0	...	...	+1·5	Faint.
	9	−3·1	...	...	+2·0	Pond's Polar Distances of 1832 have been all increased by 2″·2.
	10	−3·5	...	...	−0·5	
	11	−3·4	...	...	−0·6	
	12	−0·3	...	...	+2·2	
	13	−3·0	...	...	−0·2	
	15	−4·2	...	...	−0·1	
	17	−1·6			+0·9	
	18	+2·2	F. S.	...	+2·0	
April	7	−2·3	...	...	...	
	8	−1·5	...	...	−1·5	
	9	+1·7	...	...	−0·6	
	10	−2·2	...	...	+0·6	
	11	−2·1	...	...	+1·7	
	12	+2·0	...	...	+0·9	
	13	+0·6	...	...	−0·4	
	14	−1·1	F. S.		+5·2	
	16	+2·6	...	...	−0·6	
	17	+3·4	F. S.	...	−3·0	
	20	+0·5	F. S.	...	−0·9	
July	4	−3·2	...	...	−1·2	
	8	+3·8	...	...	+0·9	
	12	+6·3	R. ...	...	+2·6	
	14	+5·2	R. ...	...	+0·7	
	16	+5·6	...	...	+5·0	
	18	+2·4	...	...	...	
	19	(+7·3)	Extr. faint.	...	−3·1	
	20	(+10·4)	R. No star of compar.		+3·1	

Date.		$\Delta\alpha \cos\delta$.	Remarks.	$\Delta\delta$.	Remarks.
		″		″	
1838, May	2	−2·5	Very cloudy ...	+0·8	
	3	+0·1	... ...	+2·0	
	4	−1·5	... ...	−0·7	
	5	+0·4	... ...	−1·5	
	6	0·0	... ...	−1·0	
	7	+0·9	... ...	+0·3	
	8	−0·6	... ...	−2·6	
	8	...	... ...	+1·2	
	11	−2·9	... ...	−0·3	
	12	−3·7	Cloudy ...	+4·3	Cloudy.
	14	−2·2	... ...	+2·3	Faint.
	15	+1·0	... ...	+3·6	
	17	+4·4	No star of comparison.	+4·1	Very faint and flickering.
	27	0·0	... ...	−0·1	
	29	+0·6	... ...	−1·6	
	31	−0·5	... ...	−0·8	
June	2	−3·7	... ...	+1·4	
	4	−2·3	... ...	+1·4	
	6	−2·8	... ...	+3·2	North not so good as south limb.
	6	...	... ...	−0·5	
	7	−0·3	... ...	−3·3	
	8	−5·1	... ...	+0·3	
	13	−1·3	... ...	+3·9	
1843, Aug.	4	−3·4	... ..	+2·8	
	5	−2·3	... ...	−2·4	
	6	−4·2	... ...	−1·4	
	6	...	... ...	+2·4	
	7	−3·1	... ...	+1·7	
	7	...	... ...	+0·8	
	9	−2·3	... ...	+0·8	
	9	−0·6	... ...	...	
	10	0·0	... ...	+0·5	
	11	−1·7	... ...	+0·2	
	12	−2·1	... ...	+2·1	
	18	−0·5	... ...	−2·1	
	20	−1·6	... ...	...	
	22	−2·8	... ...	...	
	31	−0·5	... ..	−3·2	Merely approximative.
Sept.	2	+0·5	... ...	+0·9	
	2	...	... ...	+2·8	
	3	+0·6	... ...	+2·5	
	3	...	... ...	+2·6	
	5	−1·8*	... ...	+1·1	
	6	−3·5*	... ...	−1·1	
	6	...	* Irregularity in the	−2·3	
	7	(−8·7)*	azimuthal errors.	−2·8	
	7	...	... ...	−1·9	
	10	+2·1*	... ...	+0·8	

Date.	$\Delta\alpha \cos\delta$.	Remarks.	$\Delta\delta$.	Remarks.
	"		"	
1843, Sept. 11	−1·1	... ...	+0·7	
12	+1·4	... ...	+1·2	
16	−0·6	... ...	−0·9	
29	+0·6	... ...	−0·1	
29	...	... ...	−2·2	
Oct. 1	−2·0	... ...	+1·1	
4	−1·8	... ...	−1·1	
5	−1·5	... ...	+0·9	
9	+6·0	... ...	−1·2	
12	...	... ...	−1·0	
14	+1·8	... ...	−2·0	
14	...	... ...	−1·5	
15	+3·4	... ...	−1·7	
16	−0·6	... ...	−2·6	
17	+1·3	... ...	−2·2	
18	...	... ...	−1·2	
1844, Mar. 26	−5·4	... ...	−1·6	Both observations made with great difficulty on account of clouds.
26	...	... ...	−6·3	
28	−2·4	... ...	−2·9	
29	−2·1	... ...	−2·1	
30	−0·9	... ...	−0·2	
April 1	−0·9	... ...	+1·1	
1	...	... ...	+1·6	
2	−1·2	... ...	+0·5	
2	...	... ...	+2·5	
3	0·0	... ...	−0·5	
5	−2·4	... ...	+0·4	
6	+0·3	... ...	−2·6	
8	+2·3	... ...	+0·1	
8	...	... ...	−1·4	
9	+0·5	... ...	+2·9	
10	+0·1	... ...	+0·7	
1850, Mar. 17	−4·5	Very faint.	...	
18	−2·5	Very cloudy.	−6·0	Very faint.
25	−5·3	... ...	−0·5	
26	−4·4	... ...	+0·5	
27	−2·0	... ...	+2·6	
27	−1·7	... ...	−1·3	
28	−0·7	... ...	+3·2	
28	...	... ...	+0·8	
29	−3·1	... ...	+3·0	
29	...	... ...	+1·5	
April 2	+3·4	... ...	+0·8	
2	...	... ...	+2·3	
Aug. 13	−0·9	... ...	−1·8	
15	−0·6	... ...	+2·6	
16	...	... ...	+0·5	
18	−1·2	... ...	−1·8	

Date.	$\Delta\alpha \cos\delta$.	Remarks.	$\Delta\delta$.	Remarks.
	″		′	
1850, Aug. 18	...	... ...	−0·8	
19	+0·8	... ...	−1·9	
19	...	... ...	+3·5	
20	+4·3	... ...	+1·1	
21	+5·3	... ...	+1·9	
21	...	... ...	+0·7	
22	+2·6	... ...	+0·7	
22	+2·3	... ...	+1·9	
23	+2·8	... ...	−0·3	
23	...	... ...	+0·4	
24	+3·2	... ...	+0·6	
26	−3·0	... ...	−0·5	
28	+2·7	... ...	−0·4	
29	−1·0	... ...	−0·8	
30	−2·3	... ...	−1·5	
Sept. 11	+2·5	... ...	+3·4	
12	−0·2	... ...	−0·2	
14	+0·1	... ...	+0·6	
14	...	... ...	+1·0	
18	−0·2	... ...	...	
21	+1·3	... ...	+0·8	
21	...	... ...	+0·2	
22	+1·6	... ...	−2·2	
25	−0·8	... ...	−0·5	
26	−1·4	... ...	−1·9	
27	−2·6	... ...	−3·0	

Remarks upon the observations of right ascension for the year 1832.

' In the early part of August the pivots of the transit were examined and found to be unequally, &c.

' In the reduction no notice is taken of any instrumental errors, &c.

' The personal equation between Taylor and Ellis = 0ˢ·003, which may be neglected.

" Nothing is stated respecting the observations F S and R. In these also the personal equation would seem to have been neglected.

' July 20, faint. The observer (12) could not observe the moon-culminating stars, the atmosphere being hazy.

" If we now exclude from the foregoing observations those which I have enclosed in parentheses, since they are obviously affected with errors, and calculate the sum of the squares of the outstanding deviations from the tables, we have,—

In right ascension,

Out of 139 observations the sum of the squares of the errors = 952″·1

In declination,

Out of 157 — — — = 690′·4

Whence

The mean error of right ascension $= 2''.62$
— — declination $= 2''.10$

"If, now, in consequence of the foregoing remarks, we also exclude the observations of right ascension for the year 1832, there results in right ascension,

Out of 113 observations the sum of the squares of the errors $= 676''.2$;

consequently, the mean error of right ascension $= 2''.44$.

"These errors do not differ essentially from those which usually affect observations of the fixed stars. I may add, moreover, that it is stated, with respect to Burckhardt's Tables, that 167 observations give the sum of the squares of the errors of the tables $= 4602''$, which corresponds to a mean error $= 5''.25$.

"Besides the foregoing observations, a number of the first observations of Bradley were compared after the correction of the elements, whence there resulted the following differences between the observations and the tables:—

Date.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.	Date.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.
1751.	″	″	1752.	″	″
Oct. 24	+1·4	− 1·5	Mar. 2	−1·8	+ 4·0
25	+3·5	− 5·2	4	−1·5	+ 3·8
26	+1·4	+ 3·0	6	−6·6	+ 6·1
28	−0·3	+ 9·2	7	−3·7	+ 3·4
29	−0·4	− 0·3	7	...	+ 2·1
30	−4·3	− 0·2	9	+0·7	+ 3·7
31	−1·4	+ 1·2	11	+3·7	...
Nov. 1	−1·3	− 0·2	Oct. 11	+4·6	...
2	+7·3	− 4·0	12	−3·5	+11·2
2	...	+ 0·7	13	−2·7	− 6·0
3	+7·8	+12·1	16	−6·3	+ 2·4
7	+1·6	− 7·0	17	0·0	+ 1·2
8	+3·8	− 3·6	18	−2·5	− 1·8
9	−4·5	+ 3·0	20	0·0	− 5·6
10	+1·9	− 1·7	21	+7·9	−10·7
12	−4·1	...	23	−0·5	− 8·4
13	+3·5	...	24	−4·3	− 8·4
14	−1·8	...	25	−0·2	− 7·7
1752.			27	+5·3	− 4·4
Feb. 22	−3·9	+ 5·0	28	0·0	+ 2·4
23	−7·5	+ 2·5	29	+0·9	+ 1·8
25	+0·2	+ 3·9	30	+7·7	− 9·1
26	+0·2	+ 5·9	31	−2·4	− 4·7
27	+1·7	+ 5·3	Nov. 1	+2·6	...
28	+5·0	+ 7·7	2	+2·2	...
Mar. 1	−1·2	+ 3·8			

Date. 1753.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.	Date. 1753.	$\Delta\alpha \cos\delta$.	$\Delta\delta$.
May 6	+0″·2	+11″·9	May 15	+6″·9	+ 5″·7
7	(−10·5)	+12·9	16	−0·6	+ 6·6
8	−2·3	...	22	+3·1	+ 0·4
9	−1·5	+ 5·8	23	+0·3	+ 0·1
10	−0·5	+ 6·3	24	+0·1	− 3·2
11	+1·7	− 1·1	25	+5·2	− 4·0
12	+7·3	+ 5·0	26	+3·7	− 4·4
14	+6·7	+ 4·3			

"Excluding some observations of right ascension enclosed in parentheses, we hence deduce :—

Out of 62 observations of right ascension, the sum of the squares of the errors ..... = 884″·3

Out of 57 observations of declination ..... = 1841″·0

whence,—

The mean error of a right ascension..... = 3″·78

— — — declination .... = 5″·70

Moreover, these mean errors are not greater than one might expect of such observations, when we take into consideration the observations of the fixed stars made at the same time, and bear in mind that Bradley then employed non-achromatic telescopes and the iron quadrant.

"I have also compared with the tables a series of Dorpat Observations of the moon, which furnish very beautiful results. But as there is a circumstance connected with them with respect to which I must ask the permission of our friend Struve, I will defer their publication for the present.

"I will now give the elements of the moon's orbit which I have obtained.

For the epoch,

1800, January, 0·0^h M.T., Greenwich.

"That is to say, for mean Greenwich noon of the 31st of December of the year 1799, there were found,—

Mean longitude of the moon	=	335°	43′	26″·71
Longitude of the perigee	=	225	23	53·06
Longitude of the ascending node	=	33	16	31·15

"The following are the tropical mean motions in 100 Julian years :—

Of the mean longitude	=	307°	53′	39″·61
Of the perigee —	=	109	3	2·46
Of the node —	=	−134	8	59·61

with respect to which I would also remark, that, although I have reason to suppose that these motions of the perigee and the node very nearly represent the true values, still I do not give them as

definitive results. It is besides a very difficult matter to determine these two motions accurately to a couple of seconds, since the effects of their variation upon the moon's longitude and latitude appear to be diminishing. I will now institute a comparison between these motions and those which you have found.

"From your paper, 'Corrections of the Elements of the Moon's Orbit, &c.,' the following results are deducible :—

Motion in 100 Julian Years.

		°	′	″
Of the longitude	=	307	53	21·0
Of the perigee	=	109	3	57·5
Of the node	= −134	8	14·2	

and in the paper, 'On the Eclipses of *Agathocles*, *Thales*, and *Xerxes*,' there is to be found,

		°	′	″
Motion of the perigee	=	109	3	17·4
Motion of the node	= −134	8	54·6	

"You see from this that the motions of the perigee and node last given by you agree very nearly with the values which I have found. I must, however, add this remark, that the observations comprised between the years 1820 and 1850 bear an increase of the value of the motion of perigee found by me, less than a diminution of the same value. By such a diminution the mean errors found above might be still further diminished.

"Between the motions of mean longitude found by me there exists a greater difference, but this arises from the circumstance that I have slightly altered the coefficients of the two inequalities of long period. The accurate determination of these two inequalities by theory, is the most difficult matter which presents itself in the theory of the moon's motion. I have on two occasions, and by different methods, sought to determine their values, but I have obtained results essentially different from each other. I am now again engaged with their theoretical determination by a method which I have simplified, and hope to bring the operation to a definitive close. I have also applied to my tables some coefficients which are not free from empiricisms, but which I can justify by the circumstance that they represent the ancient as well as the modern observations with great exactness ; and it may be expected that they will represent the future observations equally well. It is, moreover, an easy matter to replace these two coefficients in the tables by others, should this be deemed necessary.

"I have found the following value of the eccentricity of the lunar orbit to be employed in the table for the equation of the centre :—

$$e = 0{\cdot}05490307.$$

"I have also found, for calculating the principal term of the latitude, the following value of the inclination :—

$$I = 5^\circ\ 8'\ 40''{\cdot}21.$$

"The coefficient of the parallactic equation I found to be,—

$$125''\cdot705;$$

an amount exceeding any which has hitherto been assigned, and which indicates a greater value of the sun's parallax than has been deduced from the observations of the transit of *Venus*. The Greenwich observations, exclusive of any others, assign the foregoing value of the parallactic inequality, and the Dorpat observations nearly the same value. I cannot, therefore, alter it. I may remark further that I have taken into consideration, not only the coefficient of the parallactic inequality itself, but also the largest of the remaining terms depending on the sun's parallax, and that the aggregate of these terms may amount to the fifth part of the coefficient of the parallactic inequality.

"The coefficients of the inequalities of the moon, depending on the [terrestrial] ellipticity, were found to have the following values:—

$$\text{In Longitude} = 7''\cdot624\,;\ \text{in Latitude} = 8''\cdot382.$$

"Add to these the terms, which I have deduced from theory, depending on the variation of the obliquity of the ecliptic, and there result in Longitude the terms,—

$$7''\cdot760 \sin(\Theta + 184^\circ\ 42') + 0''\cdot000157\,(t - 1800)\cos(\Theta + 260^\circ\ 5')\,;$$

in north latitude,—

$$8''\cdot764 \sin(v + 169^\circ\ 51') + 0''\cdot00038\cdot8\,(t - 1800)\cos(v + 278^\circ\ 39'),$$

where Θ signifies the supplement of the ascending node, and v the longitude of the moon in its orbit. The logarithm of the ratio of the moon's semidiameter, to the sine of the equatoreal horizontal parallax, was found to be as follows:—

$$\begin{aligned}\text{For the horizontal semidiameter} &= 4''\cdot750{,}484\\ \text{For the vertical semidiameter} &= 4\ 75{,}0554.\end{aligned}$$

"It is known that different telescopes assign different diameters to the stars. Now, since at Greenwich the right ascensions and declinations have been observed with different instruments, it follows that the vertical and horizontal semidiameters of the moon must be deduced independently from those data. But even in the case wherein these two polar co-ordinates have been observed with one and the same instrument—for example, the meridian circle—it becomes necessary, when the observations refer to the moon, to execute this two-fold process. For we must bear in mind that the observations are made in different ways; since the observation of declination is made by bringing the limb of the moon in contact with the horizontal wire of the instrument, or bringing it midway between two horizontal wires, while the right ascension is found by noting the times of the entrance of the moon's limb upon the vertical wires. It may very well happen, from the different modes of observing, that the hori-

zontal semidiameter may be found to be different from the vertical, even in the case wherein both of them are equal—a supposition, however, which has no foundation in theory. Nay, I have found, while engaged in determining the moon's orbit, indications that the personal equation between two observers may be different for the sun and moon from what it is for the stars, and that it even betimes may be different for the preceding and following limbs of the moon. I have not in the mean time been able to take into account this difference in the Greenwich observations, but I feel convinced that were I to do this, the agreement between the observations and the tables would be still better than it is. The difference between the above assigned values of the horizontal and vertical semidiameter is, moreover, very small, amounting only to 0″·15. Besides the above-mentioned quantities, determined by observation, which enter into my Tables of the Moon, there are two others of which I shall speak further on, after I have given some account of the mode of constructing the Tables, and of the method employed in determining the elements.

"The perturbational coefficients employed in the construction of the tables are, with the exception of the few which can only be determined by observation, those which I have calculated from theory. They are, throughout, accurate to two hundredths of a second: many of them I consider to be still more accurate. They already exhibit sensible differences from those of Damoiseau and Plana: nay, many of them differ from the corresponding terms of these tables to the extent of 2″ and 3″.* I have not yet published the perturbations of longitude and latitude, but the expression in the tables for the moon's parallax is, with a few trifling alterations, that which I made known in No. 403 of the *Astronomische Nachrichten*. The good agreement of the tables with the observations is a proof *à posteriori* of the accuracy of my perturbational coefficients, and of the soundness of the groundwork upon which the tables rest. I have applied to the latter, not only the larger coefficients, but also the smaller ones, which, indeed, are individually very trifling, but which, in their aggregate, exercise a not inconsiderable influence on the moon's place. Moreover, I have taken into account, not only the tenths, but also the hundredths of a second, since the latter produce a sensible effect upon the sum of many terms, and if they were not included, this sum, and consequently the moon's place itself, would frequently come out sensibly erroneous by the tables. Further, the secular variations of the perturbational coefficients, which are of sensible magnitude, are considered in the tables: their effect in 100 years may amount to 3″.

"The mean motions of the perigee and node above assigned are those which have been deduced from the observations, although they admit of being rigorously found by theory. But the ex-

* The sum of the differences between Plana's coefficients and mine amounts to 52″.

tremely accurate determination of them by theory, as their application to ancient and all other observations demands, is an operation of such magnitude, that it will be always preferable to deduce their values from the observations. There will, therefore, be found in the tables, with reference to these two quantities, an empiricism, similarly as in the case of the two inequalities of long period, of which mention has already been made.

"For the sake of simplicity, tables with double arguments are employed in entering the smaller terms of perturbation, and, as in the solar tables, these are so arranged, that, in practice, it will be necessary to perform only one interpolation upon each of them individually, the second interpolation being executed upon the sum of all the terms. The peculiar form of the arguments of the lunar perturbations imparts to these tables a greater degree of simplicity than that which characterises the solar tables, since, in most cases, they admit the employment of double arguments, whereby the extent of every such table is reduced to half the dimensions which otherwise it would be necessary to give to it.

"Since the most frequent use of lunar tables consists in this,—that they shall serve to compute the moon's place for a series of equal, or nearly equal, intervals of time, the arguments have been expressed, not in degrees, but in time; and since the next application of them consists in comparing with these results a large number of meridian observations of the moon, the unit assumed in the arguments is the mean time which elapses between two successive culminations of the moon, in the same way as in the solar tables the mean day is chosen as the unit of the argument. By this arrangement the comparison of the observations with the tables will be very much facilitated. In order to compare the observations of a month, we select any whatever of the mean solar times of the observations, and calculate the corresponding values of the arguments from the tables. With this argument, increased or diminished by the units, 1, 2, 3, &c., we calculate from the tables the corresponding places of the moon. These are available now for all times which approach very near the times of the moon's culmination, and by a process of interpolation which it is easy to execute, since the interpolation-factor is small, we may then transfer the calculated places to those which refer to the mean times of the meridian observations. The accuracy of the greater part of this calculation can be verified by differences, and only in respect to a small part of them will it be necessary to employ any other test. By the aid of the same tables we may also calculate an ephemeris of the moon, giving the moon's place from day to day for the same mean times as is done in the *Nautical Almanac* and other similar works. Still in this case the labour will be considerably increased, since the arguments vary throughout each day, not by a unit, but by fractions of a unit. I would prefer for such an ephemeris tables in which the unit of the argument was a mean solar day. This mode of computation is not, by any means, so laborious.

" For determining the corrections of the elliptic elements, &c., the following process was employed.—With the exception of the mean motions, these corrections were deduced entirely from the observations of our own time, the ancient observations being employed only in determining the mean motion. The reason for this mode of procedure is, that the more recent observations are available in much greater abundance than is necessary for our inquiry; that they are more accurate than the ancient observations, and, therefore, must lead to more accurate results than the latter; and that, even if the ancient observations were included in the investigation, the value of the final result would not be enhanced thereby, inasmuch as they are less precise than the modern observations.

" For every observation employed in this part of the inquiry the corresponding place was calculated from the tables, and this calculation was carried on to the observation itself. The right ascensions and declinations of the observed limbs were therefore calculated, and the corresponding observations subtracted from them, whereby the influence of the errors of the assumed provisional values of the elliptic elements, &c. was brought out. Now, since by one of the foregoing preliminary determinations, the provisional elements were very nearly correct, it was found that the differences between the computed results and the observations were already very small. In order to diminish these as much as possible, the coefficients of the variations of the elements to be corrected were calculated for each observation, and the equations of condition formed. The number of unknown quantities in these equations was eleven; but besides, the coefficients of the variations of the three mean motions were calculated, annexed to the equations, and retained in the subsequent calculations, with the view of being enabled thereby to ascertain the influence of a variation of the provisionally assumed mean motions upon the remaining unknown quantities.

" The equations of condition were now distributed in groups according to the magnitude of the coefficients of the different unknown quantities, and all the equations of each group added together.

" For example, in determining the mean longitude of the epoch, the eccentricity, and the longitude of the perigee of the epoch, the observations for every 30° of anomaly were grouped together, whereby twelve equations were obtained; and a similar process was employed for the other unknown quantities. Hence resulted a large number of equations, which were treated by a process similar to that which the method of least squares leads to. Namely, the above equations, which have been cited by way of example, were multiplied and added; first, with the coefficients of the variations of the mean longitude, then with the coefficients of the variations of the eccentricity; and, finally, with the coefficients of the variations of the perigee. Hence resulted three final equations; and the other equations, treated in a similar way, added eight

more to these; so that the number of equations was the same as that of the unknown quantities, and the values of the latter were obtained by the solution of these equations. The final equations to which one is conducted by this process do *not* possess the property that the coefficients in the vertical rows are relatively equal to those in the horizontal rows, as is the case with the final equations to which the method of least squares leads; but I can prove that they must necessarily possess this property *nearly*, and consequently that the cofficients in the rows referred to must be *nearly* equal to each other, since in each combination of groups all the corresponding equations of condition were employed. Nay, if even by this process the unknown quantities were not so determined that the sum of the squares of the remaining errors will be a minimum, still the result will be that this sum will approach very near to the minimum; and this will be sufficient in the present investigation, on which the rigorous employment of the method of least squares would entail a frightful amount of labour.

" I now come to a circumstance which has not yet been taken into consideration in the lunar theory. The remarkable circumstances which distinguish the motion of rotation of the moon necessarily imply a peculiar condition of the matter of which the moon's mass is composed; and theory replies in relation thereto, that the moment of inertia with respect to the principal axis, which is nearly parallel to the radius vector, must be the least; and that the moment of inertia with respect to the axis of rotation must be the greatest, of the three moments of inertia of the mass of the moon. The next assumption relative to the figure of the moon, and which is based upon this principle, is, that it is an ellipsoid, whose greatest axis is nearly parallel to the radius vector. Observation, however, has not hitherto confirmed this assumption. If, for example, this axis was to one of the other two in the proportion of 21 to 20, then upon this ground, and in virtue of the libration, there ought to be perceptible in the moon's semidiameter a variation of 2″. I have been unable, however, to establish any variation of this kind from the observations of the moon's diameter, which are available; and if even, in fact, a variation of the axis of the moon did exist, it must be much less than that deduced from the proportion above stated. Under these circumstances, it only remains that we assume that the interior of the moon is heterogeneous; and hence results the difference of the moments of inertia, which complicates the theory of the moon's rotation. There next arises, in connexion with this circumstance, the question whether, as in the case of the planets, the centre of the moon's figure coincides with the centre of gravity, as has been always assumed, or whether the positions of these two points may not be different? If the latter condition should really exist, then there may be found many laws of the density of the interior, in virtue of which the moment of inertia relative to the radius vector which is nearly parallel to the principal axis, will be the least of the

three, even if the figure of the moon were different from that of a sphere.

"While engaged in the determination of the elements of the moon's orbit, of which mention has been made above, I undertook to examine this question, and I have ascertained as the result of my inquiry, that the observations of the moon entirely concur in indicating a difference between the positions of the two points above referred to.

"Let α, β, γ, be the three co-ordinates of the centre of the moon relative to its centre of gravity, so that α is nearly parallel to the radius rector, β perpendicular to it in the equator, and γ perpendicular to both of these; then theory shows that α and γ may be very well determined by the observations; but β, on the contrary, can be determined only with great difficulty, or not at all. The coefficient of β is literally down to small quantities equal to the coefficient of the variation of the longitude of the perigee, and the effect of β consists, therefore in great part, only in this, that the longitude of the perigee is increased or diminished by a constant quantity. For γ the Greenwich observations give $-1''{\cdot}01$: and this quantity has been determined by itself with great certainty; only I must here remark a circumstance, which possibly may exercise an influence upon the magnitude of these quantities. It is known, that notwithstanding all the care bestowed in clearing the declinations of the stars from instrumental errors, still very frequently the declinations determined at one observatory deviate from those determined at another observatory in one direction, either north or south; and it is clear that an error of declination of this kind, should it be present, would affect the determination of γ; and the numerical value obtained for that quantity is fundamentally the aggregate of the above-mentioned co-ordinate γ and the constant error of declination.

"I must leave undecided the question, whether in the case of the Greenwich observations such an error may be supposed to exist, and leave this matter to your own judgment, since, as Director of the Greenwich Observatory, you are best acquainted with everything relative to the observations made there; but I could not refrain from a general allusion to the circumstance; nay, I even felt a desire to mention it to you.

"The co-ordinate α admits of being determined by the libration of the moon, and I have arrived at the following remarkable theorem relative to it:—

"*If the centre of gravity of the moon and the centre of its figure do not coincide, then must all the coefficients of perturbation for the mean longitude be multiplied by a constant factor, which is a function of the distance between these two centres projected upon the radius vector. If the centre of the moon be farther removed from us than the centre of gravity, then is this factor greater than unity; but if, on the contrary, the former*

be nearer to us than the latter, the factor will be greater than unity.

"The observations enable us to decide that this factor is greater than unity, and that, consequently, the centre of the moon's figure is different from its centre of gravity, and lies nearer to us than the latter. I have given myself much trouble in applying the greatest possible certainty to this determination; I have executed it in many different ways; and on each occasion I have found, from the Dorpat as well as from the Greenwich Observations, that this factor is greater than unity, even although its absolute value, which could not be otherwise expected, exhibited a slight difference as it resulted from the various determinations. The final determination gave this factor—

$$= 1{\cdot}0001544;$$

and hence there results, among other consequences, an enlargement of the coefficient of the evection equal to $0''{\cdot}69$, and it is easily recognised that the enlargement of the sum of all the perturbations may exceed $1''$.

"Esteemed Friend and Colleague! you have found by your discussion of the Greenwich Observations, extending from 1750 to 1830, that the principal coefficients of the lunar perturbations ought to be increased. You have found the increase of Plana's coefficient of the evection to be $= 1''{\cdot}28$, and the increase of his coefficient of the variation to be $= 0''{\cdot}68$; now, since the coefficient of the evection is almost double that of the variation, these enlargements seem to indicate the presence of a constant error.

"The increase of these coefficients which you have found is in each case greater than that which I have deduced, only I must here remark that Plana's coefficient of the evection is $0''{\cdot}34$ less than mine; and this circumstance leads to a closer agreement of our results. I would remark further, that you have found that Plana's coefficient of the annual equation ought to be increased by $1''{\cdot}07$, but your coefficient is too small by $1''{\cdot}1$, and therefore the extent of the increase will be considerably diminished.

"Permit me, in conclusion, to offer a few remarks on the foregoing explanation of the enlargement of the coefficients of the lunar perturbations. From the above-cited value of the factor it follows, that the centre of the moon's figure lies about 59,000 mètres; that is, about 8 geographical miles (reckoning 15 miles to a degree of the equator) nearer to us than the centre of gravity; and hence it follows, that between the two hemispheres of the moon, of which the one is directed towards the earth and the other is turned away in the opposite direction, there must exist a considerable difference with respect to level, climate, and all other circumstances depending thereon. Since the strata of homogeneous density must arrange themselves nearly in relation to the centre of gravity, it follows, if we suppose the figure of

the moon to be a sphere, that the centre of the visible disk of the moon lies about 59,000 mètres above the mean level, and the centre of the opposite hemisphere almost as much under the same level: I say 'almost,' since if, as we here must assume, the opposite hemisphere of the moon is more dense than the hemisphere turned towards us, it necessarily follows that the mean level of the former will be somewhat depressed, and the mean level of the latter somewhat elevated. If we suppose the moon to be an ellipsoid, the elongation of which lies in the direction of the earth, then the hemisphere of the moon which is next the earth will rise a little more above the mean level, and the opposite hemisphere will sink a little more beneath it. Nay, we may consider it as not impossible that the surface of the opposite hemisphere of the moon wholly or partially accommodates itself to one and the same level, in a similar way as we find to be the case with the earth.

"We need not, then, under these circumstances, wonder that the moon, when viewed from the earth, appears to be a barren region deprived of an atmosphere, and of all animal and vegetable life, since, if there existed upon the earth a mountain proportionally high, and, consequently, having an elevation of 216,000 mètres, or 29 geographical miles, there would not be recognisable upon its summit the slightest trace of an atmosphere, or of anything depending thereon. We must not, however, conclude that, on the opposite hemisphere of the moon, the same relations exist; but rather, we should expect, in consequence of the distance of the centre of figure from the centre of gravity, that an atmosphere and animal and vegetable life may there find place. Nearly at the moon's limbs the mean level must exist; consequently, we might reasonably expect to discover there some trace of an atmosphere.

"If we now inquire into the cause of this condition of the moon, I hold it to be not impossible that volcanic and other similar forces in the interior of that body may have met with far less opposition in one of the hemispheres than in the other, and may therefore have effected much greater upheavings of the surface in the former than in the latter. I am also disposed to think that the phenomena termed 'Rills,' which are perceptible on the moon's surface, and respecting which selenographers do not seem to have yet, upon the whole, arrived at any satisfactory conclusion, are rents or splits, which have been occasioned by these enormous upheavings. I submit to the judgment of astronomers these considerations, which, strictly speaking, do not belong to the theory to which this letter is devoted, but which originate in the distance of the centre of gravity of the moon from its centre of figure.

"The theory of the moon's figure, which leads to the theorem above cited, as well as to several other conclusions, has been developed by me in a Memoir which I have the honour of herewith communicating to the Royal Astronomical Society.

"*Gotha*, 1854, *Nov.* 3."

Account of the Re-mounting of his Observatory, with some Observations of the Satellite of Neptune. By William Lassell, Esq.

(*Letter to the Editor.*)

"Previous to my sending you a very small result of my astronomical labours this autumn, I may state that owing to the encroachment of buildings around my former residence of Starfield, in the vicinity of Liverpool, I have been driven two miles further into the country in the same direction, eastward of the town; and have found at least in this locality (which I consider for the present to be situated in latitude 53° 25′ 28″, and longitude west from Greenwich $11^m\ 38^s{\cdot}7$), a less smoky atmosphere, and a more rural expanse of prospect; whether a less cloudy or less disturbed atmosphere, some experience will be required to determine.

"During two or three months of the summer I was closely occupied in the removal of the two circular buildings and their domes, respectively 15 and 30 feet in diameter. The smaller dome was removed entire, on a four-wheel spring wagon. The larger was separated into four parts, as an orange is quartered, by running a saw down, so as to remove the boarding between two adjacent ribs 90° apart. Two of these quadrants, one partly placed within the other, were loaded, likewise on the same description of carriage, in a vertical position, nearly as when forming a part of the dome; and thus the whole in three journeys were safely brought to their new situation.

"Before the separation of the dome into quarters, iron plates, secured by screw-bolts, were attached across the intended sections of the base ring and of the semicircular arches which contain the grooves in which the shutters slide; and thus, when re-erected here, the replacement of the plates and bolts insured the exact juxta-position of the several quadrants. This plan has been attended with complete success, and the small portion of the removed boarding having been replaced and covered with new canvass, and the whole repainted, I believe no visible trace of its removal now remains.

"I embraced the opportunity thus afforded of improving the mounting of the dome, in respect of facility of motion, by turning a slight groove in the middle of the rim or circumference of the wheels, and introducing above and below a light curved railway bar. This has sensibly reduced the friction; and I now, without any appliance of mechanical power, can walk round with the dome, as it were, in my hand; thus carrying round with me a weight of 2400 lbs. without any inconvenient exercise of force.

"The stone piers and woodwork of both buildings, including the floors, wall-plates, and framing of lateral pulleys, were all removed with very slight derangement, and replaced in the two new circular buildings erected of precisely the same dimensions,—the very bricks of the old buildings being all removed for the

purpose. The durability of this construction of dome has been in some degree tested by this removal; for, though the smaller dome has now been erected about fifteen years, no sign of decay was discovered during its translation; even the covering of canvass requiring no repair, whilst its freedom from strain or warping during the process has been such that the quadrantal shutter slides as easily and fits as well as when in its original position.

"Since the remounting of my observatory I have found opportunity for but few observations hitherto, but having been fortunate enough to obtain a remarkably good set of measures of the satellite of *Neptune* in position and distance, I request permission to put them on record by thus communicating them to the Society.

1854, August 29th, Tuesday.

Measures of Position, with powers 366 and 614.		Measures of Distance; power 614.	
	° ′		
	202 56	0·842	revolutions.
	203 4	790	
	202 1	832	
	201 56	829	
	201 46	864	
	202 32	866	
	200 0	842	
	201 40	0·837	
Mean	201 59	Mean 0·838	
Zero	253 45	Zero −0·016	
	51 46	Hence 0·822	= 17″·86
	38 14 = angle of position at 12ʰ 20ᵐ Gr. M.T. Mean epoch of measures.		= distance at 13ʰ 5ᵐ Gr. M.T. Mean epoch of measures.

"*Bradstones, West Derby, near Liverpool, 9th November*, 1854."

NEW PLANET.

Letter from M. Chacornac to Mr. Hind, dated Imperial Observatory of Paris, October 30th, 1854.

"I have the honour to inform you that in the night of the 28th–29th of October, at 2ʰ 10ᵐ A.M., I met with a new planet; the positions of which are as follows:—

	Paris M.T. h m s	R.A. h m s	Decl. ° ′ ″	
Oct. 28	14 17 24·3	2 33 55·16	+16 58 43·7	Equatoreal.
29	12 1 14·5	2 33 8·97	+16 55 56·9	Meridian.

"This planet is the 33d; M. Goldschmidt will have announced to you to-day his discovery of the 32d."

This planet has been named *Polyhymnia*.—Ed.

NEW PLANET.

Extracts of Letters from Mr. Hermann Goldschmidt to Mr. Hind.

"*Paris, October 30th*, 1854.

"I have the pleasure to inform you of the discovery I made in the evening of the 26th inst. of a new (32) planet. It appears as a star of the 11th magnitude. I give you the approximate positions for facilitating its research, and also those of the Imperial Observatory.

Approximate Graphical Positions.

	Paris M.T.	R.A.	Decl.
	h m	h m s	° ′
Oct. 26	10 0	2 26 13	+15 5
27	8 30	25 23	15 0
	17 20	2 25 6	+14 58

Positions of the Observatory.

	Paris M T.		
Oct. 28	$13^h\ 18^m\ 11^s{\cdot}2$	R.A. = R.A. $*$ $-6^m\ 33^s{\cdot}94$	
		δ = δ $*$ $+3'\ 34''{\cdot}8$	
	Approx. Place of Star	R.A. = $2^h\ 30^m\ 55^s$	$\delta = +14°\ 51'{\cdot}5$
Oct. 29	$11^h\ 51^m\ 52^s{\cdot}0$	R.A. = R.A. $*$ $+7^m\ 1^s\ 80$	
		δ = δ $*$ $-\ 6'\ 0''{\cdot}2$	
	Approx. Place of Star,	= R.A. = $2^h\ 16^m\ 33^s$	$\delta = +14°\ 44'$
Oct. 29	$11^h\ 51^m\ 40^s{\cdot}1$	R.A. = $2^h\ 23^m\ 33^s{\cdot}03$	Meridian.

"*Paris, November 3d*, 1854.

"I certainly owe the discovery to your beautiful ecliptical charts, which have been in my possession a few months.

"Mr. Le Verrier selected the name *Pomona* for this 32d planet."

Notes on Chinese Astronomy. By John Williams, Esq.

"In presenting the Society with some illustrations of Chinese astronomy, I feel myself called upon to give some account and explanation of them.

"The first is a complete Chinese celestial atlas, in thirty-two maps. These, with one exception, have been traced from a work forming part of the Chinese Library of the late Dr. Morrison, now in the possession of University College, Gower Street, and which, by the kindness of Professor De Morgan, I have had an opportunity of examining during the past summer. This work is in 150 volumes; it is entitled 'Illustrations of the three great Principles of Nature.' These, according to the Chinese, are heaven, earth, and man. The first part of this work is devoted to astronomy. The maps have been carefully compared with those in other Chinese astro-

nomical works which have come under my observation, and also with the Chinese star-maps in the possession of the Society, and the positions of the several asterisms, ascertained as accurately as possible, by collation with European maps, and by comparing them with the 'Names of Stars and Constellations' in Dr. Morrison's Chinese Dictionary, collected for that work by John Reeves, Esq. F.R.A.S. They have likewise been compared with a catalogue in a work entitled 'Observationes Mathematicæ et Physicæ in India et China factæ a Patre Francisco Noel. Soc. Jes.,' 4to., Prague, 1710, which I have now the pleasure of presenting to the Society, and also in many instances with the heavens themselves. The results are given in the margin of each map, where will be found the Chinese name of the star or constellation in the original character, and also in English letters, with a reference to the corresponding stars in European maps. The maps are arranged according to the Chinese custom, their order being from right to left.

"The second consists of a pair of celestial planispheres of large size, published in this country about a century ago, and formerly belonging to the Mathematical Society of Spitalfields. On these I have traced in red lines the corresponding Chinese asterisms, with their names in the original characters and in English letters. The Chinese astronomers divide the visible heavens as seen in their country into thirty-one parts. Twenty-eight of these may be considered as answering to our zodiac, or, perhaps, rather to the twenty-eight lunar mansions of the astrologers. The other three are called by a word signifying a wall or enclosure, and which also implies an enclosed space. The first of these consists of the northern circumpolar stars. It comprises stars in *Ursa Minor*, *Draco*, *Cepheus*, *Cassiopeia*, *Ursa Major*, *Camelopardalis*, *Boötes*, and *Auriga*. The second comprises *Hercules*, part of *Ophiuchus*, *Serpens*, *Corona Borealis*, and part of *Boötes*. The greater portion of this is bounded by a line drawn from *β Herculis* through the head and body of *Serpens*, and the lower part of *Ophiuchus*, thence through the remainder of *Serpens* to *δ Herculis*. The third division is bounded principally by stars in *Virgo* and *Leo*, and consists of part of the constellations *Virgo*, *Leo*, *Leo Minor*, *Boötes*, *Ursa Major*, and the whole of *Coma Berenices*; I have distinguished the names of these three divisions by inscribing them within an orange-coloured line.

"The remaining twenty-eight divisions comprise the rest of the heavens. They are exceedingly unequal in extent, some containing many degrees of right ascension, others very few. They take their names from a group of stars generally situated in the centre of each division. The surrounding groups have various names, and in this respect answer in some measure to our constellations. They are, however, far more numerous. The figures of these groups, or constellations as they may be called, have no resemblance whatever to those given in European maps, the stars of each asterism being connected merely by straight lines.

"It is, however, considered that each of these twenty-eight divisions or constellations has a particular animal on earth which corresponds to it, and also that it is under the influence of one of the seven heavenly bodies, which are the Sun, the Moon, and the five planets, *Mercury*, *Venus*, *Mars*, *Jupiter*, and *Saturn*. Some authors say that twenty-eight celebrated generals, who flourished during the Han dynasty, *i. e.* from B.C. 206 to A.D. 223, were, after death, changed into these twenty-eight constellations; thus affording a curious correspondence with the ancient astronomy of the western world, in which celebrated characters are fabled to have been changed after death into constellations. The Chinese also parcel out these twenty-eight divisions into four portions of seven each. According to them, the first seven are to be reckoned from the south towards the east; the stars from which they take their names are situated in *Virgo*, *Libra*, *Scorpio*, and *Sagittarius*. The next seven are between the east and north, consisting of stars in *Sagittarius*, *Capricornus*, *Aquarius*, *Pegasus*, and *Andromeda*. Seven are situated between the north and west, comprising stars in *Andromeda*, *Aries*, *Musca*, *Taurus*, and *Orion;* and the remaining seven, between the west and south, being stars in *Gemini*, *Cancer*, *Hydra*, *Crater*, and *Corvus*. It must, however, be observed, that these divisions are not only very unequal in extent from east to west, but also from north to south, and that they differ exceedingly in the comparative number of stars in them. Thus the division *Ke* consists only of four stars in *Sagittarius* and *Telescopium*, forming the central group giving the name, and three stars in *Ara*, while the division *Tsing* consists of the whole of *Gemini*, having to the north stars in *Perseus*, and to the south stars in *Canis Minor*, *Monoceros*, *Canis Major*, *Argo*, and *Columba*. Again, the division *Tsuy* comprises the stars in the head of *Orion*, and a few to the north in *Auriga;* and the succeeding one, *Tsan*, consists of the remaining bright stars in *Orion* and stars in *Lepus*, *Columba*, and *Eridanus*. I instance these as examples of the extreme inequality of the several divisions.

"In these planispheres I have distinguished the names of the twenty-eight divisions by inscribing them within blue lines.

"In addition to the thirty-one divisions of the visible heavens, I have inserted, both in the planispheres and in the atlas, the southern stars not visible in China. These are taken from one of the star-maps in the possession of the Society; and I must here observe that, however ancient and original the northern asterisms of the Chinese may be considered, there is internal evidence to prove that these southern constellations, at least, have been derived from European sources. Thus the constellation *Phœnix* is called in Chinese 'Fire Bird;' *Dorado*, 'Gold Fish;' *Apis*, 'Honey Bee;' and so on; and the stars of each constellation are also confined to the figures, as given in our maps,—a circumstance which does not occur in the more northern asterisms.

"In tracing upon these planispheres the Chinese asterisms, every care has been taken to render them as accurate as possible. The

chief authorities from which they were taken were the Chinese star-maps in the possession of the Society. These consist of

"Two planispheres, giving the northern and southern hemispheres. Presented by Capt. Sir E. Home, Bart. R.N.

"The northern and southern hemispheres, in one sheet, with a list of the constellations and other particulars.

"An impression from the wooden block in the possession of the Society, showing the stars visible in China.

"Two planispheres in manuscript, giving the northern and southern hemispheres. On these the figures of the asterisms are very neatly drawn, and their names very distinctly written. The number of the stars in each constellation is also given, and each star has its corresponding number.

"A planisphere, showing the stars visible in China, and in a smaller circle, the southern stars not visible in the north of that country.

"The whole of these were presented by Mr. Reeves.

"The first three of these are evidently from European authorities, as divisions and numerals appear on them, which could not be derived from any other source. They are also rather meagre in names, often in many parts ill printed, and much confused in the details.

"The last two are more distinct, and appear also to be more original; and these formed the basis of the work I had undertaken, and afforded me very valuable information and assistance in it. Every group of stars in these was carefully examined; the relative position of each star determined as accurately as possible, and verified by comparison with Mr. Reeves' valuable contribution to Dr. Morrison's Dictionary, to which I have before alluded. They were also compared with the catalogue in Father Noel's work, also mentioned before. This I found of great assistance in doubtful cases. The other planispheres were also referred to occasionally; and although I cannot be satisfied that in every instance I have attained perfect accuracy, still I may confidently say that the positions assigned are, generally speaking, sufficiently correct for all ordinary purposes.

"The work by Father Noel, to which I have more than once alluded, is the third contribution to this subject. Among other things, it contains an account and explanation of the Chinese cycle of 60 years, and of the mode of its formation. It also contains a catalogue of stars, or rather asterisms, arranged according to our constellations, with the Chinese names in English characters, spelt according to the Portuguese method.

"In one part of the work the author regrets being compelled to omit the original Chinese characters for the names, &c., as he had no means of procuring the requisite types. This defect I have supplied in manuscript; and the necessity of it will be sufficiently apparent when I inform you that the Chinese vocables, being very limited, each has, in consequence, to express the sense

of many characters, whose form and meaning are totally different. Thus the characters answering to the sound of our letter E are at the least not fewer than 267, all of them distinct in their meaning, and varying exceedingly in their forms. It, consequently, follows that any attempt to ascertain the meaning of the simple vocable E occurring in a Chinese sentence written in European letters, would be almost, if not absolutely, unavailing without the original character intended to be expressed by that sound.

"In conclusion, I have only to express my hope that the pains I have bestowed on this subject will not be entirely without their use to such persons as may hereafter have occasion to investigate Chinese astronomy. At all events, they will save a vast deal of preliminary labour, which would, of necessity, be required before the party could be in a position to proceed satisfactorily with the object he might have in view. I may also state that it is possible I may, on some future occasion (should it be agreeable to the Society), enter more at large upon Chinese astronomy; and by giving an analysis of a work on this subject, published by themselves, which is in my possession, enable you to form some idea of the value of the astronomical attainments of that singular people.

"*November* 1854."

A Method of finding the Greenwich Mean Time at Sea by Computing the Moon's true Right Ascension from the observed Lunar Distance. By M. C. Rümker.

(*Communicated by the Astronomer Royal.*)

"When the observed distance of the star from the moon is small and the difference of their declination great, the error committed in determining the Greenwich mean time, by assuming the change of time proportional to that of the distance, can scarcely be compensated by the method of taking the difference of successive proportional logarithms, whereas the change of the moon's right ascension will be found more regular. I propose, therefore, to convert the observed sun's or star's distance from the moon into her true right ascension, whence the Greenwich mean time may be found by interpolation in the *Nautical Almanac* with so much the more ease and precision, as the moon's place is given there for every hour. Thus, moreover, the expense of the calculation and printing of the distances might be saved, and the volume of the *Almanac* become less voluminous.

"It remains, therefore, to be shown that the moon's true right ascension may be found with no less accuracy, and by a not more complicated calculation than the true distance.

"In order to determine the longitude, the mean time on board must be ascertained, whence the hour-angle of the star is known, and by subtracting this angle from their difference of right ascension, or adding it thereto, we obtain the hour-angle of the moon. Denoting now by α the difference of the true right ascensions of

the sun or star and the moon, by D, their apparent distance, by p, p', their true, and by P, P', their apparent polar distance; by t and $t' = \alpha \mp t$, their hour-angles; by z, z', their zenith distances; by ε, ε', their parallactic angles; by $\varrho - \pi$, or $\pi' - \varrho'$, the differences of refraction and parallax in altitude; by $d\alpha$ and dp, the effect thereof on the right ascension and polar distance; by φ', the geocentric latitude of the place of observation, lest at sea it might be judged sufficient to assume the geographic latitude for it; then the strict formula, which answers to both bodies by substituting p' for p, ε' for ε, and $\pi' - \varrho'$ for $\varrho - \pi$, when applied to the moon, is

$$\frac{\sin t \cos \varphi'}{\sin z} = \sin \varepsilon, \quad \tan (\varrho - \pi) \cos \varepsilon = \tan n, \quad \frac{\sin n \tan \varepsilon}{\sin (p \mp n)} = \tan d\alpha,$$

$$\tan \frac{(p \mp n)}{\cos d\alpha} = \tan \mathrm{P}.$$

"The lower sign applies to the moon; but if ε is obtuse the signs must be changed.

"Generally it will be found sufficient to make

$$(\varrho - \pi) \cos \varepsilon' = dp \text{ and } \frac{(\varrho - \pi) \sin \varepsilon'}{\frac{\sin (p + \mathrm{P})}{z}} = d\alpha,$$

where the following practical rule may serve as guide or confirmation.

"When the parallactic angle ε' is acute, then is

True Polar Distance { p of the sun or star $- dp$ = apparent polar distance P
p' of the moon $+ dp'$ = apparent polar distance P'.

"When the parallactic angle is obtuse the signs must be changed. Should any doubt exist whether ε is acute or obtuse, it must be found by another, to be shown hereafter. Then is, when A denotes the apparent difference of right ascensions,

$$\sqrt{\frac{\sin \tfrac{1}{2} (\mathrm{D} + \mathrm{P} - \mathrm{P}') \cos \tfrac{1}{2} (\mathrm{D} + \mathrm{P} - \mathrm{P}')}{\sin \mathrm{P} \sin \mathrm{P}'}} = \sin \tfrac{1}{2} \mathrm{A}.$$

"If, now, the sun or star, and the moon, were observed on different sides of the meridian, then is

$$\mathrm{A} + d\alpha - d\alpha = \alpha;$$

but if both bodies were observed on the same side, then is,

When the sun or star was highest $\mathrm{A} - d\alpha - d\alpha'$
moon was highest $\mathrm{A} + d\alpha + d\alpha$,

and the sun's or star's right ascension $\pm \alpha$ = moon's right ascension.

"In the following example the reduction to the geocentric zenith has been neglected.

"Sept. 30, 1857, at $11^h\ 15^m\ 54^s$ mean time P.M., being then in $16^\circ\ 29'$ south latitude and $92^\circ\ 2'$ west longitude, a set of distances

of *Jupiter* from the moon's remote limb, with the corresponding altitudes, were observed from $22\frac{1}{2}$ feet elevation above the level of the sea, the height of the barometer being $29^{in}{\cdot}6$; height of thermometer 14° Réaumur. The chronometer was $2^m\ 9^s$ slow of Greenwich mean time. The means gave

Chronometer.	Jupiter, East.	Moon.	Dist. of remote Limb.
m s	° ′ ″	° ′ ″	° ′ ″
21 54 A.M., 31 Sept., Gr.	36 32 0	68 53 37	69 26 18·2
2 9	Dip −4 42	− 4 42	Ang. Sem. − 16 16·7
24 3 Gr. M.T.	Ap. Alt. 36 27 18	68 48 55	D 69 10 1·5
	ϱ − π − 1 15·2	Moon's Ang. Sem. 16 17	
	True Alt. 36 26 3	Ap. Alt. 69 5 12	
	Z = True Zen. Dist. 53 33 57	π − ϱ + 20 34·7	
		True Alt. 69 25 46·7	
		Z′ 20 34 13	

"For the Greenwich mean time we find in the *Nautical Almanac* the sun's mean R.A. = $12^h\ 39^m\ 34^s{\cdot}6$; the moon's Declin. − 11° 8′ 45″; eq. hor. par. 58′ 37″·7; Red. − 1; the moon's semid. 16′ 0″·7; *Jupiter's* R.A. $2^h\ 51^m\ 8^s{\cdot}6$; Decl. + 14° 59′ 0″; hor. par. 2″·0; the moon's app. R.A. $22^h\ 33^m\ 34^s{\cdot}6$.

"The hour-angle t of *Jupiter*, computed from his altitude results, $2^h\ 55^m\ 40^s{\cdot}1$, which, being subtracted from their difference of R.A. $4^h\ 17^m\ 34^s{\cdot}0$, leaves for the moon's hour-angle t' = $1^h\ 21^m\ 53^s{\cdot}9$. The parallactic angle of *Jupiter* is found, ε = 55° 45′ 34″, $d\alpha$ = 64″·35, P = 104° 58′ 17″·5. For the moon we obtain ε' = 72° 40′ 26″, $d\alpha$ = 20′ 0″·9, P′ = 78° 57′ 22″·8,

	h m s
A	64 42 18
$d\alpha$	+ 1 4·35
$d\alpha'$	− 20 1·1
	64 23 21·25
α in time	4 17 33·41
Jupiter's R.A.	2 51 8·60
Comput. R.A. of Moon	22 33 35·19
Moon's R.A. at 17^h	22 32 44·37
18^h	22 34 51·12

	h m s		
	3600		Log 3·556303
Diff.	0 50·82		log 1·706035
Diff.	2 6·75		log C 7·807052
Green. M.T.	17 24 3·4		3·159390
M.T. on board	11 15 54·0		
Long. in Time	6 8 9·4	= 9° 2′ 21″ West.	

"It is true that an error in the polar distances will affect the calculation of the difference of right ascension, under the above-mentioned supposition of a small distance and a proportionally great difference of declination, more than under more favourable circumstances. But it is equally evident that a *tabular* error of declination will influence, in the same measure, the true distance in the *Nautical Almanac*, which, in that case, will be affected with nearly the whole error of the declination, whereas the influence of

any faults in the refraction and parallax, arising from erroneously observed altitudes, will remain the same, whether $\varrho - \pi$ is divided in its effects upon the distance, or upon right ascension and polar distance. Indeed, we may say the same as to the clearing of the apparent distance, when the difference of azimuths is small, and the difference of altitudes great, where then the true distance will be affected with nearly the entire errors of refraction and parallax in altitude. Hence it follows that, with respect to accuracy, there is no other difference between the two methods, than that the interpolation may be achieved more correctly by means of the right ascensions given for every hour, and that the distances may be spared. Nor offers, when the altitudes have been observed, the method proposed by me any advantages with respect to the numerical calculation. But setting aside that little dependency can be placed upon the altitudes of stars observed above the sea-horizon, on account of the variable terrestrial refraction, and that this horizon can scarcely be distinguished at night, it should be remembered that even in the day-time the altitude of the moon's crescent, when high and faint, is difficult to be observed, and that when the line joining her cusps is perpendicular to the horizon, her real upper and lower limbs are invisible; and as it may hardly be expected that the more disinterested assistants, with their not always well-adjusted quadrants, will be sufficiently attentive to the given signal, it must be owned that, if the chronometer, or approximate Greenwich time, is but known within moderate limits, the declination of the heavenly bodies is a more certain argument than their altitudes, and that computed altitudes of the moon and stars are generally better than the observed altitudes. Not to expatiate, I will only remark, that if the latitude is sufficiently known, the mean time on board may always be obtained by altitudes of the sun observed previous or subsequent to the distance, and almost independent of the latitude, twice a-day when the sun is in the prime vertical; so that, with the assistance of a good time-piece and the log, the mean time may be known at all times of the day, and be controlled by observations on the next day. It will, therefore, be advisable, when a lunar distance has been taken, at least to confirm or to correct the observed altitudes by computation, which offers the opportunity of introducing the geocentric latitude; and in that case, the method of finding the Greenwich time, by the right ascension of the moon, requires less logarithms than a correct method of clearing the distance.

"The parallactic angles and the altitudes may be found by any other formula, but I propose the following:—

$$\cot \varphi' \cos t = \tan m, \quad \frac{\cot t \sin(p \mp m)}{\sin m} = \cot \varepsilon', \quad \cot (p \mp m) \cos \varepsilon' = \tan h$$

where φ' is the geocentric latitude, m an auxiliary arc, h the altitude; the lower sign must be used when the hour-angle is obtuse.

"May 29th, 1857, in latitude 53° 33′ North, and longitude

9° 58′ 30″ East, of distances of the sun's and moon's limbs were observed, corresponding to a watch regulated to mean time. Barometer 29^{in}·9 Thermometer 15° Réaumur.

Mean of Times.	Mean of Distances.
h m s	h m s
4 39 54 P.M.	79 29 36·5
	15 36·5
	15 47·5
Appt. Dist. D	80 1 0·5

"From the *Nautical Almanac* we obtain sun's mean R.A. $4^h\ 28^m\ 29^s{\cdot}49$; sun's true R.A., $4^h\ 25^m\ 36^s{\cdot}623$; equat. of time, $+2^m\ 52^s{\cdot}9$; sun's Decl., $+21°\ 41'\ 14''{\cdot}3$; sun's semid., $15'\ 48''{\cdot}5 - 1'$ contract; moon's R.A., $10^h\ 4^m\ 28^s{\cdot}45$; moon's Decl., $+15°\ 2'\ 35''{\cdot}8$; moon's semid. $15'\ 24''{\cdot}5$ + augmen. 12″; moon's eq. hor. par. = $56'\ 24''{\cdot}8$, Red. for spheroid = 7″·2; geocentric latitude $\varphi' = 53°\ 22'\ 10''$, $t = 70°\ 41'\ 43''{\cdot}5$, $t' = 14°\ 1'\ 15''$.

"Thence we find for the sun $\varepsilon = 39°\ 55'\ 41''$, $Z = 61°\ 19'\ 29'' = \varrho - \pi$ by second approx. $= 1'\ 44''{\cdot}3$; $d\alpha = 1'\ 12''{\cdot}05$; $P = 68°\ 17'\ 26''$.

"For the moon $\varepsilon' = 13°\ 1'\ 40''$; $Z' = 39°\ 53'\ 8''{\cdot}5$; $\pi' - \varrho' = 35'\ 45''{\cdot}5$; $d\alpha' = 8'\ 19''{\cdot}48$; $P' = 75°\ 32'\ 14''{\cdot}3$; A $84°\ 50'\ 9''{\cdot}0$; $\alpha = 84°\ 43'\ 1''{\cdot}57$, in time $5^h\ 38^m\ 52^s{\cdot}105$, which, being added to the sun's true R.A. $4^h\ 25^m\ 36^s{\cdot}623$, gives for the moon's true R.A. $10^h\ 4^m\ 28^s{\cdot}728$, whence the longitude will be found 9° 58′·24."

On the Connexion between the Sun's Motion and the Variations of Terrestrial Magnetism. By Professor Secchi.

(*Extract of a Letter to Capt. Manners, R.N.*)

"Colonel Sabine, as you know, has published and discussed with admirable sagacity an immense mass of observations of the phenomena of terrestrial magnetism, and in the last volume of the the Toronto Observations has shown that in the tropical regions the opposition of oscillation of the needle follows so strictly the change of the sun's declination, that the change of the former can be traced to the very day of the equinox. This consequence, although apparent from the curves of the tropical countries, was not so clearly perceived in the countries more distant from the tropics. But it was evident, that if the law was true, it would become visible everywhere, provided a suitable mode of analysis should be applied. I thought, therefore, that the want of its being apparent there, was due to the kind of graphical analysis used until now, which consists in referring the monthly curves, either to the absolute mean of the year, or to the mean of the month itself. I undertook, therefore, to refer the monthly curves to the mean annual hourly curve of the year. The reason of doing so is this: the effect of diurnal variation at any hour (as far as it depends on the sun) is the complex effect of its hour-angle and of declination. The mean annual hourly curve, as deduced from the observations of the whole year, is a residual function independent of the declination which acts in opposite directions in

the two halves of it. Therefore, if from any mean monthly curve we subtract the mean hourly curve of the year, the residual quantity so obtained will be a function of the declination of the sun. I have, therefore, executed these operations (which are done very easily, using the tables and plates of Colonel Sabine exhibiting the monthly ranges of the needle variation in declination), and the result has been a good deal more satisfactory than I ever expected. The results for the four colonial observatories are contained in the figures here enclosed, and a simple glance at them shows that for opposite declinations of the sun their curvatures are directed in opposite parts, and that their change can be traced to the very day of the equinox. Even some irregularities which may puzzle in the figures of Colonel Sabine for the tropical regions, have their explanation here. Thus, for instance, at St. Helena the curve of June has a descending flexure which is wanting in that of January. Now the differential curves of the declination show that this branch disappears from the interference of a similar branch, but opposed in the two months of opposite declination. We can generally state, that the mean curve of the declinometer at any country of the globe results from the interference of the two curves, one depending only on the diurnal period, and the other on the declination of the sun: the first is constant in its direction through all the year, and the second changes according to the position of the sun relatively to the equator. The inspection of these curves shows a general feature of the solar periodical influence: this is, that the maximum and minimum differ in time very nearly six hours; and their phases depend on the passage of the sun through the magnetic meridian of the place, so that an acceleration occurs in those places where the north declination is greater. For the southern hemisphere the complete antagonism of forces requires a contrary law.

"These facts seem to strengthen the opinion that the solar action is a direct one on the needle. To verify this hypothesis, I have subjected it to calculation in this way: I consider the needle, according to the Anpérian theory, as an element of galvanic current circulating during the day in a circle, which is the parallel that it describes during the diurnal rotation of the earth; and applying to this hypothesis the formula given by Savary for the action of a magnet on an element of current circulating in a circle, neglecting the terms of the order higher than the first and those depending on the parallax of the sun, I obtain the three following expressions for the components of the solar action, relatively to the horizon as a fundamental plane of co-ordinates:

$$X_0 = \frac{k\,l\,R}{r^2} \sin\delta \sin\omega,$$

$$Y_0 = \frac{k\,l\,R}{r^2} \cos(L+\delta)\cos\omega,$$

$$Z_0 = \frac{k\,l\,R}{r^2} \sin(L+\delta)\cos\omega,$$

and the resultant,

$$S = \frac{k\,l\,R}{r^2}\sqrt{1-\cos^2\delta\,\sin^2\omega}.$$

Where X_0 is the component directed perpendicularly to the meridian along the line east-west; Y_0 that directed along the line south-north, and Z_0 is vertical; k is a constant co-efficient dependent on the intensity of solar force; l, the length and magnetical moment of the needle; r, the distance of the sun from the centre of the circle; R, the radius of the parallel described by the needle; L, the latitude of the place; δ, the declination of the sun; ω, its hour-angle. If now we call Δ the deviation of the needle from its mean position, and P the resultant of those forces which act in that plane in which the needle is only moveable, we will have

$$\Delta = \frac{P}{\tau}\sin h + 2\,\frac{P^2}{\tau^2}\sin 2h + \ldots .$$

where τ is the component of the force of terrestrial magnetism or gravity, which acts in the plane of P, and h the angle between P and the mean position of the needle; and also here we will retain only the first term.

"To find the different laws of motion for the three well-known magnetical instruments, let us suppose first a place where the declination of the needle $= o$: in this case the magnetic and astronomical meridians coincide; and for the declinometer we shall have $P = S \cos b$, b being the altitude of the sun, and calling a its azimuth, it will be

$$\Delta = \frac{k\,l\,R}{\tau\,r^2}\sqrt{1-\cos^2\delta\,\sin^2\omega}\,\cos b\,\sin a,$$

and since

$$\cos x = \cos b\,\sin a = \sin\omega\,\cos\delta,$$

we have

$$\Delta = \frac{k\,l\,R}{2\,\tau\,r^2}\sin 2x,$$

calling x the angle made by the sun with the first point of east. Likewise for the bifilar magnetometer we have

$$\Delta b = \frac{k\,l\,R}{\tau\,r^2}\sin x\,\cos y,$$

y being the angle between the sun and the fourth point of the horizon; that is, the angle between the sun's radius vector and the axis $o\,y$. Finally, for the vertical-force magnetometer, in the prime vertical

$$\Delta_0 = \frac{k\,l\,R}{r^2}\sin x\,\sin y\,\sin h',$$

the angle h' being given from the equation $\tan h' = \cot b\,\sin a$. These formulæ give for the declinometer a periodical variation,

which is double during a day everywhere. But for the bifilar magnetometer they show that on two occasions its period, which is generally also a double, is reduced to a simple one, viz., when $L = o$ and $\delta = o$. The vertical-force magnetometer, also, has a simple period at the equator complementary to the bifilar, as appears clearly from the value of

$$P = \sqrt{X^2 + Z^2}$$

which for $L = o$ is reduced to

$$P = \frac{k R l}{r^2} \sin \delta,$$

which multiplied by sin h', gives a simple period. In any other place the period is always double. These conclusions agree with the Observations, and explain the march of the components of horizontal and vertical force at St. Helena; and they exhibit the simple period of the bifilar magnetometer at the equinoxes, which is sufficiently well proved from the observations. These formulæ give for the day and night equal excursions, because they make abstraction of the influence of the interposition of the terrestrial globe during the night-time. This being magnetical, diminishes strongly the solar influence. Hence, the night excursions exist, but exceedingly much diminished in extent. We can apply the same formulæ to this case also, if we suppose the distance of the needle much increased, so that the night-range should become $\frac{1}{6}$ or $\frac{1}{7}$ of the day-range, as is manifest from the figures given by Sabine, *Hob. Observ.* (vol. i. p. 48, plate.) This also may explain some irregularities in the day-period; since a period begun, when the earth is not interposed (the needle not being yet in the shadow), will be partly destroyed by the supervening discontinuity of action. This, practically, must be very often the case, since the co-ordinate axes, which have been supposed to be directed according to the fundamental points of the astronomical horizon, must be rather directed towards the cardinal points which refer to the magnetic axis of the earth, and then w must be considered and computed from the sun's passage at the magnetic meridian. Our formulæ may explain also the singular fact of irregular perturbations of the needle happening at certain fixed hours. This phenomenon, although supposed to be connected with meteorological causes, is evidently also in connexion with solar periods, so that the maxima of perturbation arrive at the hours of maxima and minima of nocturnal periods. This is the case with the nine o'clock perturbations. To explain all the phenomena of diurnal variation is certainly an attempt superior to the state of our actual knowledge of magnetism, since this explanation depends upon the law of distribution of magnetism on the earth and the sun, and upon the law of magnetical induction, both which are completely unknown to us; what I have exposed here is only to try if, by means of the known laws of nature, we can thrown any light on this difficult subject. It is obvious that I do not mean to exclude

variations of magnetism due to meteorological influences and auroras, or to any other cause of irregular perturbation, since I have taken into consideration only the effects of the sun, and even neglected some small parts and coefficients, which may become sensible perhaps in individual cases, and explain minor periods.

"*Roman College Observatory,*
"*Rome, Sept.* 9, 1854."

On Saturn's Rings. By J. R. Hind, Esq.

"In the *Monthly Notice* of this Society for June 1853, is given an extract from a letter of Sig. Secchi's, addressed to Capt. Manners, R.N., referring to a drawing of *Saturn's* ring, executed by Campani at Rome in 1664, and considered to afford confirmatory evidence of that remarkable conclusion deduced by M. Otto Struve, — that the ring is contracting upon the ball at a rate which at no distant period may bring it into contact with the globe.

"I have not seen it noticed anywhere in print, that the first volume of Lubienietski's *Theatrum Cometicum* contains a well-engraved sketch of the appearance of *Saturn* and his ring, no doubt founded upon the drawing, the original of which was forwarded by Signor Secchi to Capt. Manners.* It accompanied a letter from Father Athanasius Kirch to Lubienietski, dated Rome, 1665, July 25, and is thus alluded to in that communication,— '*Mitto hisce inclusum Systema Saturninum à Campano ingeniosissimo Telescoplopœo* 50 *palmorum tubo observatum.*' It appears to have been taken at the end of July 1664, when the ring was very open. On measuring the relative breadths of the ring and the dark space between the interior edge and the ball, the latter is seen to be very slightly the broadest. Reducing the dimensions to seconds of arc with M. Otto Struve's value for the equatoreal diameter of the globe, I find for the breadth of the rings 5″·58 and of the dark space 5″·77. These numbers agree very well with the relative breadth inferred from a remark of Huyghens, in his *Systema Saturnium*, applicable to the year 1657, — that the breadth of the interval between the interior border of the ring and the body of the planet was equal to, or perhaps rather exceeded, the breadth of the ring. M. Otto Struve argues that this observation of Huyghens, when taken in comparison with recent measures, is alone sufficient to prove that a very considerable change has occurred in the system of the rings since the middle of the seventeenth century: Campani's drawing is certainly rather favourable than otherwise to this view.

"The roughly executed diagram of *Saturn* and his ring, at the time of the greatest inclination of its plane to our line of vision in July 1667, found in Lemonnier's *Histoire Céleste*, —

* See *Monthly Notice* for June 1853.

though apparently less deserving of confidence than that given by Lubienietski, may be worth mention, as the evidence it affords, whether reliable or not, tends to support M. Otto Struve's inference. In this sketch the breadth of the dark space, compared with that of the rings, is about as 3 to 2. The diminished breadth of the space in the diagrams which follow may be accounted for by the closing up of the rings and the imperfections of telescopes in those times. It should be remarked, however, that Picard's avowed object in presenting this drawing, taken in July 1667, was to point out the extension of the ring beyond the edge of the globe, and had no reference to the comparative breadths of the ring and dark interval.

"I have found amongst Picard's observations on the ring, in the same work, a notice of that second dark belt, traversing the globe at the interior edge of the rings, which, with every appearance of probability, has been identified with the obscure ring of modern observers. M. Otto Struve alludes to observations of this nature made early in the eighteenth century; but probably Picard's had escaped his notice. Under date 1673, June 15, it is remarked, '*Saturne étoit sorti des raïons du soleil: il y avoit deux barres noires qui marquoient les deux bords intérieurs et extérieurs de l'anneau.*' A rough sketch is annexed to illustrate the observation. It appears difficult to account for this belt on any other assumption than the above; and we have, therefore, reason to suspect the existence of the obscure ring, at least as far back as 1673, notwithstanding the negative evidence which the subsequent observations of Sir W. Herschel and others may be considered to afford.

"At page 25 of the *Histoire Céleste* are some measures of the diameter of the ring by the same observer in the years 1666 and 1667, and approximate determinations of the angle of position of the greater axis with respect to the meridian. On the following page are also measures of the diameter of *Jupiter*, between October 3d, 1666, and November 10th, 1667; yet, strange to say, Picard does not appear to have remarked the compression at the poles during the interval over which these measures extend, since, in a note dated 1673, April 13, he says, 'I began to perceive that the disk of *Jupiter* was a little oval, and that the greater diameter is always in the direction of the belts.' It is uncertain, therefore, in what direction the planet was measured; but to obtain something like a probable value of the major axis of *Saturn's* ring in 1667, I have compared the measures of *Jupiter* with the mean results of Struve and others, assuming that Picard's numbers represent the *mean* diameters of the planet, and thus have deduced a correction to be applied to the measures of *Saturn*. In this way I find the exterior diameter of the ring = 44″·1 (for *Saturn's* mean distance). If we compare with the values given by the observations of diameter with the meridian instruments at the Royal Observatory, this number would be increased to 47″·1. In either case it is larger than the measures of Bessel, O. Struve, and

others, now indicate, — a mean of their results being 39″·5 at the same unit of distance. So far, then, as Picard's observations may be considered deserving of reliance, there is evidence that the exterior diameter of the ring is diminishing, with the gradual approach of its inner edge to the ball: the rate of contraction being apparently about 2″ in a hundred years. In saying thus much, however, I am well aware that great uncertainty attaches to any conclusion that we can arrive at on this subject, with the data at present before us."

Those who are interested in the curious theory respecting *Saturn's* rings, recently propounded by M. Otto Struve, and who are of opinion that the observations of the seventeenth century may be to some extent legitimately employed in testing that theory, will find an original representation of the planet and its appendage by Campani, in the MS. *Letter Book* of the Royal Society (vol. i. p. 324). This drawing, which appears on a small slip of paper carefully inserted in the volume, accompanies a letter from Auzout to Oldenburg, in which the writer states that it had been forwarded by Campani himself in a letter to Cassini, who was then residing at Paris. It is dated October 5, 1665. There are also two drawings of *Saturn*, as seen by Cassini in the years 1676 and 1677, to be found at page 584 of vol. x. of the *Hist. Anc. Acad. des Sciences.* It may be remarked that the interesting observation of Picard's respecting the spheroidal figure of *Jupiter*, which is cited by Mr. Hind in the foregoing paper, is the earliest *distinct* recognition of a real ellipticity of any of the celestial bodies which is to be found in the records of astronomical science, as the writer of this note has recently shown, it is to be hoped satisfactorily, in a paper *On the Early History of the Researches of Astronomers relative to the Spheroidal Figure of the Earth*, which is inserted in the *Monthly Notice* for June of this year (vol. xiv. p. 232), and to which the reader is referred for various other original notes and observations of a similar kind to that of the French astronomer. The remark of Picard is the more creditable to the acuteness and sagacity of that admirable astronomer, when it is considered that hitherto no other individual, with the exception of perhaps Cassini, appears to have entertained the remotest suspicion of the ellipticity of the planet, notwithstanding that it had been the object of continual scrutiny by observers since the invention of the telescope about the beginning of the seventeenth century. As regards Cassini, it may be stated, that, down to the year 1690, he had not entertained *a decided conviction* of the ellipticity of the planet, since in that year we find him still impressed with the idea that its disk was quite round, as is evident from the following passage extracted from the Registers of the Academy of Sciences of Paris for the year 1690 :—" *M. Cassini remarqua à cette occasion que Jupiter, qui lui avait parût autrefois d'une figure un peu ovale, dont le*

plus grand diametre tendait d'orient en occident lui paraissait à present parfaitement rond."—(*Hist. Anc. Acad. des Sciences*, vol. ii. p. 108.) In the very next year he announced definitively the spheroidal figure of the planet, and assigned to the ellipticity a value which appears to be about as good as any that has since been given.—EDITOR.

First Elements and Ephemeris of Polyhymnia,

Computed by Mr. George Rümker, from the following Observations :—

Paris, Oct. 29, 1854.
Berlin, Nov. 3,
Durham, Nov. 9,

Mean Anomaly	36° 2′ 51″·0	1854, Nov. 10·0, G.M.T.
Perihelion	325 13 54·0	Mean Eq. 1854, Jan. 0·0
Node	13 55 38·3	
Inclination	2 27 59·1	
Angle of Eccentricity	23 46 42·9	
Log. a	0·505894	
Log. μ	2·791166	

These elements represent the middle observation with errors,—
(Calculation − Observation) + 0″·2 in Longitude, 0·0 in Latitude.

The foregoing elements were accompanied with an Ephemeris of the planet, extending from Nov. 14 to Dec. 16.

Mr. E. B. Powell writes, that in his paper on the elements of Comet II. 1854 (see *Monthly Notices*, vol. xiv. p. 221), he unfortunately, by an oversight, inserted wrong numbers for the errors of anomaly. The following are the correct numbers and the resulting elements as now furnished by him :—

$\nu - u = -45''$ for the error of anomaly between the 8th and 13th
$\nu' - u' = -11''$ — — — 8th and 18th
$\nu'' - u'' = +(1' - 30'')$ — — — 8th and 27th

Taking the means of the results afforded by the angles for the two latter intervals, the following elements are obtained :—

Time of Perihelion Passage ..... = March 24·03468
Perihelion Distance ..... = ·2783
Inclination of Orbit ..... = 81° 44′
Longitude of Perihelion on Ecliptic... = 170 16
Longitude of Ascending Node..... = 316 0
Motion retrograde.

At the close of the meeting the President alluded to the curious theory which had been recently started, relative to the

probability of a slow change continually going on in the position of *Saturn's* rings with respect to the body of the planet ; and he pointed out the advantage of examining for this purpose the early observations of the rings, similarly, as on the present occasion, Mr. Hind had done in the paper which had just been read before the Society. He also expressed the interest which he felt in the paper communicated by Mr. Williams, illustrative of some points in Chinese Astronomy, remarking that while such researches were highly creditable to their author, they could not fail, if carefully prosecuted, to be attended with advantage to astronomical science. He then stated that there were three other circumstances to which he would allude in a few words. In the first place he would remark that the results of the Greenwich observations were now published in a separate form, and might be obtained by any person upon applying for them by letter addressed to the Royal Observatory. Secondly, he wished to direct the attention of observers to the fact that, on the Monday following (Nov. 13), the planet *Saturn* would approach very near a small star. He remarked that it was highly desirable to watch such close junctures, inasmuch as the observations of them might occasionally serve to throw light on the physical constitution of the rings of the planet. There was only one recorded instance of a star having been seen in the dark space between the rings and the body of the planet, but the evidence, in support of that observation, could not, perhaps, be regarded as sufficiently worthy of confidence. The President finally adverted, in very brief terms, to the series of pendulum experiments which had been recently executed under his superintendence at Harton Colliery, near South Shields, with the view of obtaining data for ascertaining the mean density of the earth, reserving, for a future occasion, a more detailed account of the operations. He alluded to the attempt of one of his predecessors, Dr. Maskelyne, to accomplish the same object by means of experiments on the deviation of the plumb-line from the vertical direction, occasioned by the attraction of the mountain of Schehallien, in Perthshire. He also noticed the experiments for the same purpose with the torsion balance, adding, however, that he did not consider them to be sufficiently trustworthy, in consequence of the numerous disturbing influences to which operations of so delicate a nature are liable. He then adverted to the experiments with the pendulum, which his friend, Dr. Whewell, and himself had made in the years 1826 and 1828, in the bottom of Dolcoath mine, Cornwall, with the view of obtaining a solution of the same important problem. Notwithstanding several obvious disadvantages under which they laboured, they persevered in the pursuit of their object, until their labours were definitively interrupted by the flooding of the mine. He remarked that in conducting experiments of this nature, there were three essential points to be attended to.—First, there was the comparison of the two pendulums (one of which was to be used at the bottom of the mine and the other at the top) in order to ascertain their physical difference: this was satisfactorily effected by an interchange of the

positions of the two pendulums, any difference in their rate of oscillation being thereby completely eliminated. Secondly, there was the comparison of each pendulum with its clock ; this object was attained by employing the method of coincidences originally suggested by Captain Kater. Thirdly, an accurate comparison of the clock at the bottom of the mine with the clock at the top was indispensable. This was by far the most difficult part of the operations. In the Dolcoath experiment this object was effected by the very inconvenient process of carrying chronometers from the top of the mine to the bottom along a series of ladders. The application of galvanism to astronomical observations had suggested to the President the advantage of comparing the two clocks by effecting a galvanic connexion between them. This is accordingly the method which has been practised in the Harton Colliery experiments : it would be hardly necessary to add that it was, in every respect, superior to any other mode of comparison. The President finally remarked, that until all the necessary calculations were worked out, it was impossible to ascertain the definitive result of the experiments.

Note on the Origin of the Attempts made in the seventeenth Century to derive from physical Principles an invariable Standard of Measure. By the Editor.

The only physical standards of measure, which have been applied to astronomical and other scientific purposes in modern times, are those which have been furnished by the oscillations of the pendulum and the length of an arc of the meridian. This note is accordingly confined to some remarks on the origin of these two methods.

The pendulum seems to have *first* suggested itself to scientific men as capable of furnishing a permanent standard of measure. Sprat, in his History of the Royal Society (1667), enumerating the inventions and discoveries due to one or other of the Fellows of the Society, mentions the invention of "*a universal standard or measure of magnitudes by the help of a pendulum, never before attempted.*"—(4th Edition, p. 247). In a subsequent part of the same work the author gives a special account of the labours of Dr. (afterwards Sir Christopher) Wren, in the course of which he states that the latter had shown "*that there may be produced a natural standard for measure from the pendulum for vulgar use.*"—(p. 314.)

I am not acquainted with any author who has since alluded to the labours of Wren or any other Fellow of the Royal Society in connexion with this subject. The primary idea of employing the oscillations of the pendulum to obtain a standard of measure is not generally adverted to by scientific writers. Delambre, in his Analysis of the *Horologium Oscillatorium*, pub-

lished in 1673 (*Hist. Art. Mod.* vol. ii.), notices the proposal of Huyghens to establish an invariable standard of measure on the basis of the oscillations of the pendulum; but he does not assert that the suggestion of such a measure was due to the Dutch philosopher. On the contrary, he refers to a somewhat similar proposal made by Mouton in his work — *Observationes Diametrorum Solis et Lunæ*, Lugd. 1670. It has, however, been recently asserted, although in somewhat obscure terms, upon the strength of the passage in the *Horologium Oscillatorium*, above alluded to, that the invention is really due to Huyghens. This point will be best ascertained by citing the words of the author himself. Referring to the advantage of a knowledge of the centre of oscillation, in finding the length of the pendulum, he thus remarks:—"Hinc necesse fuit illis, qui, ante hanc centri oscillatorii determinationem mensuræ universalis constituendæ rationem inierunt; *quod, jam inde à prima Horologii nostri inventione, nobilis illa Societas Regia Anglicana sibi negotium sumpsit* et recentius doctissimus Astronomus Lugdunensis, Gabriel Moutonus; his, inquam, necesse fuit designare globuli suspensi diametrum, vel proportione certa ad fili longitudinem cujus nempe tricessimam vel aliquam partem æquaret."— (*Horologium, Pars Quarta*, Prop. xxv.) It appears clearly from the foregoing passage that the idea of deriving an invariable standard of measure from the oscillations of the pendulum originated with the Royal Society, having been suggested to some of the Fellows by Huyghens' application of the pendulum to clocks.

Having recently had occasion to examine the MSS. of the Royal Society, I availed myself of the privilege so liberally accorded to me by that body, to ascertain if any records existed of the early labours of the Society in connexion with the establishmont of an invariable standard of measure. In this research I was not disappointed. The following extract of a letter from Huyghens to Sir Robert Moray, dated the Hague, Dec. 30, 1661, confirms the statement above cited from the *Horologium Oscillatorium:* "La fabrique de ma machine [pneumatique] m'a empeché quelque temps de travailler aux traités dont vous me demandez de nouvelles. J'ai entre les mains celui de l'horloge, duquel une grande partie est dediée aux mouvements, *et particulièrement j'y ai parlé de cet usage du pendule pour la mesure universelle, dont vous dites qu'on a traité dans votre assemblée.*" (*Letter Book*, vol. i. p. 22.)

Upon referring to the *Journal Book* of the Royal Society, I found that the question of an invariable standard of measure, founded on the oscillations of the pendulum, had been discussed at several of the meetings of the Society, towards the close of the year 1661 and the beginning of the following year. At the meeting, held on the 22d of January, 1662, it is stated that the President, Lord Brouncker, "introduced the history and schemes of the pendulum experiment, and that a committee, consisting of the President, Mr. Boyle, Sir William Pettey, Dr. Wilkins, and

Dr. Wren, was appointed to make trials of it." (*Journal Book*, vol. i. p. 46.)

It would appear that the experiments of the Committee were not successful, for, at the meeting held on Feb. 5, 1662, "*Dr. Wren was entreated to think of an easy way for a universal measure, other than the pendulum.*" (*Journal Book*, vol. i. p. 48.)

Viewing the statement of Sprat, in connexion with this passage, it appears extremely probable that Wren was the first who proposed the deriving a standard of measure from the oscillations of the pendulum. The idea was, indeed, worthy of the genius of that distinguished man.

The difficulties experienced by the Committee, doubtless, arose from their ignorance of the centre of oscillation of the different pendulums, as well as from the disturbing effects of the resistance of the air and various other causes.

At length the discovery of a method for determining the centre of oscillation by Huyghens, removed what was considered to be the sole impediment to the success of the pendulum experiment in furnishing an invariable standard of measure. Huyghens announced his discovery in a letter to Sir Robert Moray, dated Nov. 21, 1664. He gives the rule for finding the centre of oscillation of a pendulum, consisting of a homogeneous spherical ball, suspended by a thread or wire. He states that for a pendulum oscillating half-seconds, he found by calculation that the distance between the point of suspension and the centre of oscillation was exactly $9\frac{1}{2}$ inches, Rhinland measure. (*Letter Book of the Royal Society*, vol. i. p. 225.)

The letter of Huyghens, above referred to, was read at the meeting of the Royal Society, on the 23d of November, 1664. In the *Journal Book* it is simply recorded that the letter was read; but there is evidence to show that the discovery, announced by Huyghens, was justly appreciated by the Society. Oldenburg, in a letter to Boyle, dated Dec. 3d, 1664, states that when the letter of Huyghens was read to the Society, "order was given that it should be punctually registered, to the end that Mr. Huyghens might have his due, and his inventions recorded for his honour to posterity." (Boyle's Works, vol. v. p. 328.)

The Society lost no time in verifying the theorem of Huyghens, respecting the centre of oscillation, by making experiments with pendulums of different lengths, and employing oscillating spheres of different magnitudes. In the first experiments with pendulums, oscillating in half-seconds, the theorem of Huyghens was found to "*be very near the truth.*" In the second series of experiments, when the oscillations of the pendulum were performed in seconds, it was found on one occasion that the string, with the smaller sphere attached to it, was $\frac{1}{10}$ *longer than it ought to have been, according to Huyghens' theorem.* A similar result was obtained in other instances wherein spheres of different magnitudes were employed. This inconsistency between theory and the results of actual experiment was attributed to the resistance of the air.

Thus, notwithstanding the beautiful discovery of Huyghens, respecting the centre of oscillation, it was found that the oscillations of the pendulum could not be confidently employed in furnishing an invariable standard of measure. Hooke, who, probably, felt somewhat piqued, on account of the small share which he had in the inquiry, was the most strenuous objector to the use of this method. He remarked, among other disadvantages which would accompany its practical application, that if, as was in all probability the case, the force of gravity increased in intensity from the equator to the poles, it would necessarily follow that *in proceeding from the equator towards either of the poles, the oscillations of the seconds' pendulum would gradually be quickened.* This suggestion, so characteristic of the genius of Hooke, was announced to the Royal Society nine years previous to the return of Richer from Cayenne.*

Mouton (*Observationes Diametrorum*, &c., 1670) appears to have first proposed the length of an arc of the meridian as the basis of an invariable standard of measure. In the Appendix, p. 427, he suggests a decimal system of measures, the fundamental unit of which he proposes to derive from the value of a minute in Riccioli's length of a degree of the meridian; and he finds the unit of measure † thus obtained to be equal to the length of a simple pendulum, which makes 1252 oscillations in half-an-hour.

I am indebted to Mr. Sheepshanks for the foregoing statement respecting the proposal of Mouton to derive a standard of measure from the length of an arc of the meridian, not having myself seen the work in which reference is made to that method. ‡

It was soon suspected, as in the case of the pendulum, that the method of deriving a standard measure from the length of an arc of the meridian might be liable to serious objections. At the meeting of the Royal Society, held on the 7th of June, 1682, when the question of measuring an arc of the meridian in England was discussed, the President (Sir Christopher Wren) remarked that, according to his opinion, the best standard of measure "would be a certain part of the length of a degree upon the earth, *if at least upon several accurate trials of the measure of a degree in several latitudes it should be found the same, and not different, as it would be if the body of the earth were oval and not perfectly globular.*" (*Journal Book*, vol. vii. p. 86.)

It appears somewhat strange that it did not occur to any one that the supposed defects of the methods based upon the pendulum and the length of an arc of the meridian might be obviated by confining the experiments and observations to a fixed latitude.

* Hooke's objections to the use of the pendulum, in furnishing an invariable standard of measure, were stated at the meeting of the Society, held on Dec. 14, 1664.

† Termed by him *the Virga*; it was equal to the thousandth part of the length of a minute of Riccioli's arc.

‡ Delambre has justly remarked that Mouton's work is very rare.

The planet (31), discovered at Washington, U.S., by Mr. Ferguson, on September 1, has been named by him *Euphrosyne.*

The *Nautical Almanac* for the year 1858 has recently been published. In addition to the usual matter there is a Supplement, containing approximate Ephemerides of the newly-discovered planets, down to *Amphitrite* (29), for the year 1855, and also an ephemeris of De Vico's Comet for the same year. The Supplement also includes numerical values of the elements of the four minor planets, — *Thetis* (17), *Lutetia* (21), *Calliope* (29) and *Amphitrite* (29).

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